The evolution of the Kydland-Prescott research program: transformation, recollections and new documents

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**Introduction**

There are a number of retrospective narratives regarding the development of the path-breaking works of Kydland and Prescott (1977, 1982) for which they received the Nobel Prize in economics. These include the personal recollections of Prescott (1998, 2004, 2006e), Kydland (2005, autobiography) and Lucas (2001, 2005), and the survey of the Royal Swedish Academy (2004). While important in their own right, the history of the Kydland-Prescott research program is even more intriguing than that recounted in these narratives, especially when the development of their ideas is dealt with in detail by combining the recollections and documents that characterize their evolution.

The approach taken here both complements and supplements these narratives. The published accounts of Prescott, Kydland, and Lucas mentioned above are supplemented via additional recollections collected --by means of correspondence and in-depth background interviews-- from them and others who influenced and were influenced by the Kydland-Prescott research program, *while detailed textual analysis of drafts of papers involved in its evolution complements the narrative recollections*. Moreover, as Hicks put it, since "memory is treacherous" (1973, 2), utilization of these drafts can clear up minor anomalies in the narratives themselves.

Some examples should suffice to illustrate the efficacy of the approach taken here. In his insightful Prize Lecture, Prescott mentioned that he and Kydland "had read the Lucas critique", and searched for the "best rule to follow"--from the context of the paragraph where the account appears--as early as 1973 (2004, 374, 2006e). But in the references, only the 1976 version of the Lucas critique is cited (1976a), although in additional recollections, both Prescott and Kydland recalled reading and being influenced by the 1973 *drafts* of Lucas.
Indeed, just how and to what extent they were influenced will be recounted below. According to Prescott, a "key" element in the evolution of the Kydland-Prescott approach was the development by Lucas and Prescott of "recursive competitive equilibrium theory" in their seminal 1971 paper "Investment under Uncertainty" and in Lucas (1972), and its further development "in Prescott and Mehra (1980)" (2004, 392, 2006e). But while the difference between the published and draft version of Lucas and Prescott is mentioned by Prescott in a note (2004, 392 note 15, 2006e), the December 1977 draft of Prescott and Mehra is not, leaving the impression of a long gap in the evolution of the theory. Moreover, the cardinal role of the 1971 Lucas-Prescott paper (written in 1969) and how it changed Prescott's approach to macroeconomics, will be shown by using the information he provided in correspondence; while other key episodes in the evolution of the Kydland-Prescott approach will also be enriched by utilizing correspondence and interview material as supplementary narrative. Finally, in his retrospective "recollections" (2005) Lucas posed the crucial, albeit up to now unanswered question regarding the 1982 Kydland-Prescott paper: "How did they ever think to put all these pieces together in just this way?" (2005, 777).

The object of this study is to show how Kydland and Prescott put all the "pieces" together; that is to say, how their overall approach, which, as will be shown, encompasses both their 1977 and 1982 papers, evolved, and brought about what Prescott, in his Nobel lecture, called "a transformation" in modern economic analysis. This is done by:

(i) surveying their early work on optimal policy rules and stabilization, and dynamic equilibrium models of the business cycle;
(ii) analyzing the impact of those "pieces" which catalyzed their approach, such as the early drafts of the Lucas Critique (1973) and Hodrick-Prescott (1978);

(iii) integrating this with the evolution of their approach based upon the variorum drafts of their 1977 "Rules" and 1982 "Time to Build" papers and the relationship between these papers;

(iv) unifying the accounts and narratives, by means of in-depth questioning as to the intellectual process involved in their path-breaking work.

Now, in his Prize lecture, Prescott wrote (2004, 370, 2006e) "all stories about transformation have three essential parts: the time prior to the key change, the transformative era, and the new period that has been impacted by the change".

But what is the "transformation" that Kydland and Prescott brought about? It is not simply the synthetic combination of the work of Frisch, Solow, Lucas, and others. It is a totally new approach, one that extends the work of these giants, in conjunction with a new economic weltanschauung and empirical methodology that has brought about a sea-change in macroeconomic research and policy analysis.

The "essential parts" in the context of this paper are, then, as follows. In the first part of the paper, the "pre-dynamic general equilibrium" phase of the early work of Prescott and Kydland, over the period 1967-1973, is surveyed. The second part of the paper deals with the "transitional" phase in the Kydland-Prescott approach, encompassing dynamic equilibrium models, their "search" for rules, and their work on stabilization policy, over the period 1973-1978. The third part of the paper examines in detail the evolution of their work on business cycles, including the "empirical investigation" of Hodrick-Prescott and "Time to Build" and their aftermath, covering the period from 1978-1982 onwards.
Before the transformation: the pre-dynamic general equilibrium tradition and the Lucas Critique

Prescott's early contributions

In his Nobel lecture, Prescott described the nature of macroeconomic models and policy discussion prior to what he called the "transformation", saying that he had "worked in this tradition" and went on to outline the approach to policy selection taken in his dissertation (2004, 372-73). Prescott's early published contributions (1971, 1972), along with his collaboration with others (Lovell and Prescott, 1968, 1970; Lucas and Prescott, 1971) are important for understanding of how his thought evolved. Prescott's 1967 Carnegie Tech dissertation was entitled "Adaptive decision rules for macroeconomic planning" and was supervised by Lovell. It dealt with the optimization problem with special emphasis on the issue of how uncertain parameters affect decisions. Prescott considers that his dissertation "was in the old tradition" (2005a). According to Lovell (2005a), the thesis "was concerned with a problem of optimal learning while doing. Theil was using macro econometric models he had estimated in a control theory framework to determine optimal fiscal policy for the next period. As each new observation became available with the passage of time Theil would re-estimate the model, and then work out the optimal policy under the assumption that the policy parameters had been estimated with precision. Among other results, Ed showed that one could do better than successive one-period optimizations, in terms of minimizing loss, if one sacrificed a little of next-period optimization of the control problem in order to design a better experiment which would yield more precise parameter estimates that would more accurately guide future policy decisions."
There are two important points in Lovell's description of Prescott's 1967 thesis, that of control theory and experimentation respectively; for these aspects are key issues in the work that emanated from his thesis, and collaborative work between them during the period. The first of these was a joint paper with Lovell published in *Southern Economic Journal* (1968), originally presented at the December 1964 meeting of the Econometric Society (1968, 60).

Almost four decades later, Lovell and Prescott provided recollections of the central message of their 1968 paper. According to Prescott (2005 a), in the paper, they "broke from treating the equations governing the evolution of the national account statistics as data tradition whom... we had rational expectations with regard to "desired capital stock" and examine the mapping from policy rules to statistical properties of the time series". In his retrospective assessment of the paper, Lovell wrote (2005 a): "I would put a rather different spin on it. Our paper challenged the assertion that the Fed's actions were necessarily destabilizing if it allowed the money supply to move procyclically. To show this in as simple a framework as possible we introduced money into the multiplier-accelerator model of the business cycle. We made the money supply endogenous, the Fed adjusting the money supply in response to movements in GNP. The interest rate was influenced by M, and investment depended in part on the rate of interest. We then showed that one could not say without knowing the parameters of the system what value of the policy parameter would best smooth the cycle (i.e., yield the smallest characteristic root). We then added stochastic shocks to the system and found that the value of the policy parameter minimizing the variance of output could not be specified *a priori*. Obviously, this is not RBC Prescott. He had not yet liberated himself from my influence."
To this, Prescott added (2005 b): "I am in basic agreement with Mike on the SEJ paper. But there the idea of evaluating a policy rule by looking at operating characteristics of the model (the model was not an economy so I do not say model economy) was a different way to think about things. Also in the investment equation, there was a future value of a variable. This means that expectations as to this variable had to be formed. One expectation scheme we considered was rational expectations. Except for this the model was in the pre dynamic general equilibrium tradition."

In his recollections, Lovell also described the impact of his 1970 paper with Prescott on his own view of econometric results (2005 a): "Ed and I wrote a second paper, "Multiple Regressions with Inequality Constraints: Pretesting Bias, Hypothesis Testing and Efficiency," (JASA 1970). We found that dropping variables with incorrect sign from a multiple regression would lead to biased estimates of the parameters of the variables remaining in the model; although the estimates would be efficient if the stochastic disturbance was normally distributed. Worse, the t statistics could be grossly exaggerated. Partly as a result of this paper, I became more and more disillusioned about the validity of econometric results."

It is not surprising, then, that Prescott would also turn away from empirical econometrics, when this outcome was combined with the early impact of the Lucas critique (1973) on him, as will be seen below.

At this point, a caveat is necessary. Due to its importance as the turning point in the evolution of his thought--on his own account, as will be seen below--Prescott's 1971 paper with Lucas "Investment under uncertainty" will be dealt with after the papers emanating directly from his 1967 dissertation.
Prescott published a paper with the same title as his thesis in the December 1971 issue of Western Economic Journal. This paper cites both the unpublished thesis and his forthcoming paper "The multi-period control problem under uncertainty". Both papers indicate that Prescott was starting to think about alternative methods of analysis, albeit still in "the old tradition". In his 1971 paper, Prescott introduced the "concept of experimentation" as an "additional element" (1971, 369-70) and "backward inductions and numerical methods" for two-period analysis (1971, 370-372). He noted that "a more complete analysis of the multi-period control problem" could be found in his forthcoming *Econometrica* paper (1971, 372 note 9; 1972). In the final part of his 1971 paper, Prescott utilized, as a baseline, the small-scale Keynesian econometric model formulated by Chow (1967) to simulate the U.S. economy and assess the outcome of linear as against adaptive--and both against perfect information--decision rules. He found that the outcome of the testing procedure he used clearly demonstrated that the adaptive approach gave superior results in the context of the economies simulated. Prescott concluded that additional research was required, including "how best to approximate policy makers preference ordering using a quadratic function" (1971, 374-78). The importance of this will be seen below.

Prescott's paper "The multi-period control problem under uncertainty" appeared in the November 1972 issue of *Econometrica*. It was also presented in May 1972 at the first Optimal Control Conference (Chow, 2005). The manuscript was received in December 1970 and its revision in June 1971 (1972, 1057). This paper constitutes perhaps the earliest application of multi-period control theory to economics (Prescott, 1972, 1043 note 2; Kendrick, 2005 a, 18). In this paper, he analyzed the control problem by applying numerical methods and showed, among other points, that "the
more periods remaining in the planning horizon, the more important is experimentation" (1972, 1056). Indeed, as Kendrick recently wrote in correspondence (2005 b) "Prescott's 1972 paper was one of his most important contributions and one that has not received the attention it deserves".

Lucas and Prescott published their paper "Investment under uncertainty" in the September 1971 issue of *Econometrica*, although the work had been completed in 1969. This paper, according to Prescott, was also in the pre-transformation tradition. In his words (2004, 373, 2006e) "the macroeconometric models organized the field. Success in macroeconomics was to have your equation incorporated into the macroeconometric models. Indeed, Lucas and I were searching for a better investment equation when in 1969 we wrote our paper"

But more is involved here than simply the search for a "better investment equation". And indeed, in subsequent correspondence, Prescott stated just how his paper with Lucas changed the direction of his thought. As he wrote (2005 d):

"Investment under Uncertainty was the paper that led me to work on dynamic equilibrium models of business cycles. After writing that paper in 1969 (it appeared in 1971 after a very long delay subsequent to acceptance), I stopped teaching macro. Another approach was needed. Finn and I developed the needed approach".

**The Lucas Critique and its impact on Prescott and Kydland**

At this point, the role of the Lucas Critique has to be taken into account. However, before assessing its impact on Prescott and Kydland, the evolution of this watershed paper itself must be dealt with. In his Nobel lecture, Prescott indicates that he and Kydland had read the Lucas Critique paper as early as 1973 (2004, 373-74, 2006e). There are, in fact, two drafts of "Econometric policy evaluation: A critique". The first is dated April 1973, and was prepared for the Phillips curve conference,
University of Rochester, 20-21 April; the second is the May 1973 revision of the April 1973 paper, which was the version eventually published in 1976. There are differences between the April and May drafts of the Critique, such as changes in the model in the section entitled "Taxation and investment demand", inclusion of responses to discussion at the Rochester Conference, and a specific point made by Prescott. In the April 1973 draft, the section on taxation and investment contains an approach based upon a "standard accelerator model of investment behavior with a cash flow expression incorporating the tax structure, following Jorgenson [1963]", and aggregated from the firm to industry level (Lucas 1973 a, 17). The May 1973 revision, which was eventually published in 1976, also has a section entitled "Taxation and investment demand", but the approach is that of a standard accelerator model of investment behavior, based, in part, on Hall and Jorgenson (1967). In recent correspondence, Lucas recalled (2006) that "the later model is an improvement... the problem here was exposition: How to explain what the point was simply". In the acknowledgements on the title page of the May draft, Lucas thanked Prescott, among others, for "helpful reactions to an earlier draft of his paper". Lucas recalled (2006) that "It is hard to isolate Ed's influence. He and I had working out the theory of investment together long before this, so all my thinking on investment was influenced by him. Note 16 in the May version is certainly a response to Ed: He had kidded me about being careless about time units earlier: "If you want a big effect, why not measure time in seconds?"

Moreover, as Lucas also recalled (2006), the concluding paragraphs in the section on Phillips curves in the May draft were "probably added on in response" to the Rochester discussions. In the April 1973 draft, Lucas concluded this section by writing "Evidently, the actual [his emphasis] consequences of an increase in \( \pi \)
(that is an increase in the average inflation rate...) will have no relation to the long-run prediction based on [equation] (22)” (1973 a, 25). In the May 1973 revision, Lucas added two paragraphs to the end of this section. In these he stated the central message of his Critique regarding empirical Phillips curves: first, that the "long run... relationship as calculated or simulated in the conventional way has no bearing on the actual consequences of pursuing a policy of inflation"; second, that "empirical Phillips curves will appear subject to "parameter drift"... unpredictable for all but the very near future" (1973 b, 29).

Interestingly enough, the importance of detailed textural analysis can also be seen in January 1976 Econometrica paper of Cooley and Prescott, "Estimation in the presence of stochastic parameter variation". A close inspection of this paper shows that the manuscript was originally received in August 1972, before the presentation of the April 1973 draft of the Lucas Critique. The "last revision" was received in November 1974 (1976, 180). This explains the inclusion, in the text of the central message of the Lucas Critique (1976a, 167). The reference, however, is not to the published version of the Lucas Critique, but to a "Carnegie-Mellon working paper, 1973" (1976a, 183).

The Lucas Critique impacted on Prescott, for one, even before the April 1973 draft, on Prescott's own account, for as he recalled (2006 a) "When Bob discussed with me the theme of Econometric Policy Evaluation, (which was when he was orchestrating the theme in his paper) the importance of his insight did not hit me. That was in 1972. As soon as I saw the paper, it hit me and hit me hard." In further correspondence, he wrote (2006 c) "When in 1972 he pointed out to me that the equations of the macro econometric models were not policy invariant, I did not realize the importance of the point. After hearing the Critique presented and reading
one of the versions, I realized the importance of the point. We had to do something different to evaluate policy. I did see how to evaluate policy rules in theory at least after hearing the Critique. I did not consider the details of the examples important. The Critique led me to conclude that econometrically, something had to change. Eventually I came to the conclusion that we had to organize our empirical knowledge around preferences and technology, that is people's willingness and ability to substitute and not around equations. Given the policy rule and preferences and technology, economists should compute the equilibrium law of motion for that policy rule."

Moreover, in his Nobel lecture, he wrote (2004, 373, 2006e): "A key assumption in the system-of-equations approach is that the equations are policy invariant. As Lucas points out in his critique... this assumption is inconsistent with dynamic economic theory. His insight made it clear that there was no hope for the neo-classical synthesis-that is, the development of neo-classical underpinnings of the system-of-equations macro models. Fortunately, with advances in dynamic economic theory an alternative set of tractable macro models was developed for drawing scientific inference. The key development was recursive competitive equilibrium theory in Lucas and Prescott (1971) and Lucas (1972). Equilibrium being represented as a set of stochastic processes with stationary transition probabilities was crucial to the revolution in macroeconomics."

The evolutionary story of the overall Kydland-Prescott approach, however, does not end with Lucas and Prescott (1971) and the Lucas Critique (1973); rather, it starts there. In correspondence, Prescott recalled (2001): "Kydland in his dissertation (1973)... extends recursive methods to...class symmetric dynamic games. This formulation is exploited in Kydland and my paper "Rules rather than discretion: the
time inconsistency of optimum plans" (1977a), written 1975 while I was visiting the Norwegian School of Business and Economics) and in my and Rajnish Mehra's paper "Recursive competitive equilibrium" (1980). Kydland, for his part, described his 1973 Carnegie-Mellon thesis entitled "Decentralized macroeconomic planning", which was supervised by Prescott, as placing emphasis on Stackelberg dynamic games, the time inconsistency issue, and player dominance, with the fiscal policy maker dominant, and the monetary policy maker the follower (interview, 2005).

However, in order to fully comprehend the evolution of the Kydland-Prescott approach, we must now turn to the phase of transition and transformation, that is, from 1973 to 1978, which encompasses their "search" for dynamic models and rules in order to evaluate policy. For as Prescott said in correspondence (2005 d) "Lucas's Critique did influence Finn and me to search for optimal policy rules. This is discussed in my Nobel address". And indeed, in it he wrote (2004, 374, 2006e): "Finn and I had read the Lucas critique and knew that for dynamic equilibrium models, only policy rules could be evaluated. This led us to search for a best rule to follow, where a rule specifies policy actions as a function of the state or position of the economy. We had worked on this problem before Finn left Carnegie-Mellon to join the faculty of the Norwegian School of Business and Economics in 1973. In academic year 1974-1975 I visited the Norwegian School of Business and Economics, and in the spring of 1975 Finn and I returned to this problem. This is when we wrote our paper "Rules rather than discretion: the inconsistency of optimal plans... ". Kydland and Prescott, then, had read Lucas Critique in 1973, and by 1975 had written a first draft of their "Rules vs. Discretion" paper. But before this, they had to throw off what they considered to be the intellectual blinder of "optimal control ". 
The Transitional phase: dynamic equilibrium models, rules and stabilization policy

As Prescott wrote in his Nobel lecture (2004, 373) "before the transformation, optimal policy selection was a matter of solving... a control problem... the optimal policy is time- consistent, and dynamic programming techniques can be used to find the optimal policy. This is true even if there is uncertainty in the model economy". And, in a series of papers, Kydland and Prescott individually, and jointly-- Prescott with Cooley, as noted above, and then Mehra-- made the transition from the pre dynamic general equilibrium "systems of equations" approach to the post transformation approach as manifest in their 1977 and 1982 papers.

Control vs. Game theoretic approaches to stabilization

The first of the joint Kydland-Prescott papers influenced by the April 1973 Lucas Critique was their 1973 paper "Optimal stabilization". The first draft of the paper was presented in June 1973 at the NBER- NSF Chicago conference on stochastic control and economic systems with the subtitle "a new formulation". As Kydland recalls (2005 a) "Ed and I…[were] influenced by a paper by Lucas in writing our paper for the June 1973 conference in Chicago at which we presented the first draft of the paper, which we started to work on in April 1973 (I remember mainly because we started just before I defended my dissertation)…". As Chow recalls (2005) "There were a number of papers presented in that conference by (at least by now) well-known economists, including Ed Burmeister, J Philip Cooper, Richard Cyert, Richard Day, Morris DeGroot, Ray Fair, Stan Fischer, David Kendrick, Robert Holbrook, Michael Intrilligator, Morton Kamien, Robert Pindyck, Gordon Rausser, Steve Ross, Michael Rothschild, Nancy Schwartz, Chris Sims, and John Taylor, among others. The paper
by Kydland and Prescott did not seem to stand out among some of these other good papers". In the 1973 paper, as Kydland later wrote (1975, 334) "the problem of finding optimal stabilization policies for a competitive economy was formulated as a dominant player stochastic game. The policymaker is the dominant player, taking into account the reaction functions of economic agents. The results were found to have important implications for econometric policy evaluation". A second version of the paper, with the subtitle "a new approach", was presented at a conference on modeling and simulation in May 1974, and what would seem to be an abridged version published in the conference proceedings (1974). Besides the methodological impact described by Prescott, Kydland also recalled the expositional impact of Lucas's April 1973 draft, writing (2005 a): "that paper by Bob already included an investment-tax-credit example, and Ed's and my key example, in both our 1973 paper and later in the rules vs. discretion paper, involved investment tax credit."

As will be seen below, Kydland's recollection is confirmed by reference to Prescott's unpublished April 1974 paper "Money, expectations and the business cycle". This paper, "written for discussion only" (Prescott,1974) was presented by Prescott "in the fall of 1974" at the Norwegian School of Business and Economics, which, as noted above, he visited in 1974-75 (2005 e). On the title page, Prescott said that the paper represented "work in progress". In the introduction, he outlined the "operational framework" (his emphasis) he wanted to develop. This involved the utilization of "a dynamic general equilibrium framework, in its true sense"(1974, 1). Prescott went on, in a note, to refer to both the 1973 version of Lucas's Critique, and also his 1973 paper with Kydland, cited above (1974, 2). As he wrote "Kydland and Prescott [1973] used this approach [i.e. the analysis and evaluation of "policy rules" (his emphasis) which specify, as he put it "a vector of policy variables... as a function
of state variables (and possibly lagged variables)] to evaluate investment tax credit policies ..." (1974, 2).

But again, more is involved here than simply the confirmation of recollections. For Prescott's April 1974 paper can be said to have set out a dual purpose research agenda. This is seen in his statement of "goals" and again in his summary and conclusions. As he put it, these goals were "(1) to develop a theory of the business cycle which is a competitive equilibrium... and (2) to develop operational procedures to evaluate alternative stabilization policy rules" (1974,19). He also talked about the relationship between the "business cycle application" and a "full employment path, which can be determined using optimal growth theory" (1974, 18) and the need for "methods... to compute the competitive equilibrium"(1974, 19). He then presented as an example of the proposed approach, a model including a production function with a technology shift parameter, capital stock equations, a utility function for preferences, policy functions and an objective function; the model also included state and decision variables (1974, 20-21). He went on to say "there is a need for developments which permit the direct calculation of the competitive equilibrium for structures of reasonable complexity" (1974, 24). Finally, Prescott concluded (1974, 25) "In summary, this is but a first step towards the development of a theory of the business cycle and an operational framework for correctly evaluating stabilization policy. Much research remains to be done". But this task would only be made easier by resolution of the issue of how, and whether, "control theory" could be applied to the problem of economic stabilization.
Kydland's early contributions

In a series of conference presentations and published papers based upon his own 1973 dissertation "Decentralized macroeconomic planning", over the period 1974-1977, Kydland dealt with, among other issues, the question of whether "decentralized policy-making" could be considered "as a dynamic game"; for example, in his June 1975 IER paper (1975, 334) (received March 1974, revised August 1974).

The same year, 1975, according to his own recollections, Kydland submitted his "assignment problem paper ["Decentralized stabilization policies: optimization and the assignment problem"] to a stochastic control conference to take place in Boston in May" (Kydland, Nobel autobiography, 2005). According to the conference program as reported in the Spring 1976 issue Annals of Social and Economic Measurement, this was the paper that was listed in the May 1975 conference program (Chow, 2005). And indeed, the "assignment problem" paper was published in 1976 in the Annals.

However, as Kydland recalled, "At some point early in the conference, Gregory Chow announced a session for work in progress. I signed up to talk about Ed's and my paper, and was told I could go first. All hell broke loose. Everyone was trying to locate the error [time inconsistency]. Admittedly, we had chosen a rather provocative title for our first draft: "On the inapplicability of optimal control for policymaking". I was certain nothing was wrong. With all my experience in dynamic dominant-player games, I knew time inconsistency had to be an issue. I suppose at that point, after what happened at that presentation, I realized our findings could generate considerable attention. Moreover, as a consequence of the difficulty people had in understanding the time inconsistency, we decided to add, for expository reasons, a Phillips-curve example to our investment-tax-credit example before we submitted our revised version of the paper to the Journal of Political Economy. As I recall, it was
motivated by a model in a recent paper by Phelps and Taylor. Of course, that example has turned out to be used a lot by subsequent writers" (Kydland, Nobel Autobiography, 2005).

Kendrick, for his part (2005 a, 15) recalled that Kydland's "talk at the meeting was well attended and listened to carefully". According to Kendrick, this talk, in conjunction with the Lucas Critique, brought about a situation in which " work on control theory models in general and stochastic control models in particular went into rapid decline and remained that way for a substantial time...the work on uncertainty (other than additive noise terms) in macroeconomic policy mostly stopped and then slowly was replaced with methods of solving models with rational expectations and with game theory approaches" (2005 a, 15). Kendrick qualified this (2005, 15 note 6), however, by referring to exceptions in papers by Turnovsky and Brock (1980, 1981).

In March 1975, while at the Norwegian School of Economics and Business Administration, Kydland circulated a discussion paper entitled "Equilibrium solutions in dynamic dominant-player models" which was eventually published in JET in August 1977. Kydland referred to this paper in his 1976 paper "Decentralized stabilization policies: optimization and the assignment problem", published in Annals of Social and Economic Measurement, as noted above, and this discussion paper was also referred to by Kydland and Prescott in their June 1977 JPE "Rules vs. Discretion" paper.
Rules vs. Discretion, time-to-build, stabilization, control, recursive competitive equilibrium and the business cycle

The "key example" in their seminal 1977 paper, as Kydland put it, was the "investment-tax-credit example". What is important to realize is that this example encompasses a "two-period time to build approach" reflecting "the fact that time is required to expand capacity, and investment expenditures occur over the entire time interval" (1977, 482), as recognized by both Kydland (2005, interview) and Prescott (2006d). This is a crucial point in the evolution of the Kydland-Prescott approach, for it illustrates the inherent linkage between their 1977 and 1982 paper.

In recent correspondence (2006d), Prescott has also stressed the dynamic general equilibrium nature of the "Rules vs. discretion" paper. After acknowledging that the investment-tax-credit example did "exploit the rental price of capital theory of Jorgenson and of Jorgenson and Hall", he continued on to say "but what is important is that it exploits the theory developed in "Investment under uncertainty" to derive the equilibrium process given the policy rule. Unlike Bob's [Lucas] "Neutrality" paper, there is capital accumulation so it was truly dynamic. Lucas comes up with the mapping from an investment tax policy rule to the equilibrium process of the economy …Finn and my analysis introduces maximizing households, so we have a dynamic general equilibrium analysis".

In 1977, Prescott also published a paper entitled "Should control theory be used for economic stabilization?" in the Carnegie-Rochester series. In this paper, Prescott concluded that not only should control theory not be utilized for stabilization, but "until a tested theory of the business cycle is available, it is best that active stabilization not be attempted" (1977, 33). Modigliani and Taylor, among others, commented on the paper. Modigliani was critical of what he called "Prescott's
provocative paper", but still said that it had "sensitized" him "to a potentially important problem" (1977, 98). Taylor, for his part, made the point that in his paper, Prescott had distinguished between "optimal control theory" which "is inappropriate for stabilization problems" and "optimal design theory", which, according to Taylor, on Prescott's view "should be used to find policy rules which generate the best operating characteristics for the economy". Taylor's conclusion was "not that optimal control theory is inappropriate for stabilization problems but simply that its incorrect use is inappropriate" (1977, 94). In a rejoinder, while disagreeing with a number of issues raised by Modigliani in his critique, Prescott reiterated what he took to be "the major point" of his own paper, which he said is that "if dynamic economic behavior (e.g. fluctuation in economic activity) is viewed as an equilibrium phenomenon, with agents forecasting efficiently conditional on their information sets, then optimal control is inappropriate" (1977, 101).

In December 1977, Prescott and Mehra circulated a draft paper entitled "Recursive competitive equilibria and capital asset pricing" (Prescott and Mehra, 1977). In 1978, this paper was revised and given the title "Recursive competitive equilibrium: the case of homogenous households", and circulated as a Columbia University Graduate School of Business working paper. The paper was eventually published in the Sept.1980 issue of Econometrica. The 1978 draft of the paper was received in December 1978, and the final corrected version in December 1979 (1980 a, 1378). In their 1977 draft, Prescott and Mehra further developed the recursive competitive equilibrium framework originally presented in Lucas and Prescott (1971). Moreover, they extended it to the analysis of the cases of "many consumers" and "small fluctuations in aggregate output". In the former case, their analysis was of "an economy with many consumer classes, where each class has different preferences, but
the same discount factor" (1977, 21). The latter was an analysis of the case where "fluctuations in aggregate output are but a few percent"(1977, 22). They concluded (1977, 23) "These difficult and important extensions and applications will be the subject of future inquiry within our recursive competitive equilibrium framework".

The former case is an important one, since--based upon the 1977 draft, as cited above--if all agents have the same discount rate, and if conditions satisfy that a competitive equilibrium Pareto Optimum is ensured, then, as Prescott and Mehra later wrote (1980 a, 1365) " equilibrium processes for economic aggregates and prices [for some heterogeneous consumer economy] will be observationally equivalent to those for some homogeneous consumer economy". Moreover, that fluctuations in aggregate output, is mentioned in their 1977 draft is also significant, although there is no mention of this in the 1980 version. The importance of the 1977 and 1978 drafts of Prescott and Mehra lies not only in it being the linkage between Lucas and Prescott (1971) and their 1980 paper, but because of their impact, it would seem, on Long and Plosser's 1983 paper, that is to say, on the 1980 draft of Long and Plosser, as will be shown below.

In their 1983 JPE paper, Long and Plosser wrote (1983, 43 note 4): "The model we employ is quite similar to the model described in Prescott and Mehra (1980). Their remarks (p. 1365) about the identical consumers assumption (i.e. it is not quite as restrictive as it may appear) and their treatment of the optimality of competitive equilibrium are particularly relevant. They do not, however, explicitly consider the business-cycle implications of their models". Moreover, in correspondence with Mehra (2005 a, b, c), he acknowledged that Long had seen the 1977 version of Prescott-Mehra at a Rochester "job seminar" that took place in "late 1977 or early 1978". The Prescott-Mehra paper was published in Sept. 1980. Now, the earliest
citation of what was to become the Long-Plosser paper this author has found dates to 1980, as cited in the August 1980 draft of the now classic "Two Charlie's paper" (Nelson and Plosser, 1980). This implies that unless either Long or Plosser, or both, were referees of the 1980 Prescott-Mehra paper-- which is doubtful-- it can be surmised that Long and Plosser most probably extended the 1977 and 1978 Prescott and Mehra approach.

**Enigmas and Variorum Drafts: the Kydland-Prescott NBER Conference paper**

The transitional phase in the evolution of the Kydland-Prescott approach reached its ultimate stage with the presentation of a paper by Kydland and Prescott at the 1978 NBER conference on rational expectations and economic policy. The story surrounding this watershed paper is enigmatic, to say the least. This is because those who attended the conference, and commented on the Kydland-Prescott paper, did not realize its significance, as will be seen below; although this is not unique, as the same phenomenon occurred when Muth's original Rational Expectations paper was presented at an Econometric Society meeting in December 1959 (see Young and Darity, 2001) Moreover, the evolution of the 1978 conference paper itself, from its initial form, through the draft presented at the conference, to its final published version in the 1980 NBER Conference volume, is a key element in the "Time to Build" story.

In order to understand the importance of this paper in the ongoing intellectual process that culminated in the 1982 "Time to Build" paper, however, we must first turn to how Lucas--and Kydland—respectively perceived what occurred at the conference where the Kydland-Prescott paper on "Stabilization Policy" was given. There are two versions of Lucas's recollections regarding the NBER conference held at the Bald Peak Colony Club, New Hampshire, October 1978 (Fischer, 1980). In his
"Professional Memoir", Lucas wrote (2001, 28) " At that conference, Ed Prescott presented a model of his and Finn Kydland's that was a kind of mixture of Brock and Mirman's model of growth subject to stochastic technology shocks and my model of monetary shocks. When Ed presented his results, everyone could see they were important but the paper was so novel and complicated that no one could see exactly what they were. Later on, as they gained more experience through numerical simulations of their Bald Peak model, Kydland and Prescott found that monetary shocks were just not pulling their weight: By removing all monetary aspects of the theory, they obtained a far simpler and more comprehensible structure that fit postwar U.S. time series just as well as the original version. Besides introducing an important substantive refocusing of business cycle research, Kydland and Prescott introduced a new style of comparing theory to evidence that has had an enormous, beneficial effect on empirical work in the field".

Lucas published "Present at the creation: reflections on the 2004 Nobel Prize to Finn Kydland and Edward Prescott" in the *Review of Economic Dynamics* (2005). Lucas wrote (2005, 777) "The first public presentation of "Time to build..." occurred at an Oct. 1978 conference sponsored by the Federal Reserve Bank of Boston. You might picture the scene as something like the New York appearance of King Kong, when the theater curtain is drawn and the 40-foot ape is revealed, struggling with his chains. But it was nothing like that. The paper …was too hard to be read in advance, and Ed's presentation was technical and confusing". He continued (2005, 778) "I should say that this paper was not the version that was published in Econometrica in 1982. The 1978 version had a kind of nominal wage stickiness, related to my 1972 information-based model (Lucas 1972). This feature is now interesting mainly as evidence that Ed and Finn did not start out by attempting to show that business cycles
were real in origin or that monetary influences were unimportant. Their substantive aims at the time were pretty standard. But their methods were brand new, and it was only after much experimentation with the model that they were led to the discovery [his emphasis] that the real, technology shocks were doing all the work, and the sticky wage part was contributing nothing".

In his Nobel autobiography, Kydland also recalled events surrounding the 1978 NBER conference paper. As he put it (2005, autobiography): "For an NBER conference in 1978, we wrote a paper that was somewhat schizophrenic. It contained a business cycle model, but also evaluated stabilization policy. The main idea behind the latter was that changes in taxes were costly as a way to balance the government budget over the cycle. Instead the "slack" should be picked up by fluctuations in government debt. In the end, we were asked to reduce the length of the paper for the resulting conference volume published by the NBER in 1980, and we had to leave out much of that material".

Detailed comments on the Kydland-Prescott conference paper were made by Feldstein, Hall, and Taylor (1980, 187-194). The general discussion appearing in the volume also cited comments by Blinder and Nelson, among others. Recollections of some of those who attended the conference, and also of those who commented on the paper, are now presented, so as to gauge the reactions to the paper then, and now.

Blanchard, who attended the conference, recalled (2002) that he did not think that the Kydland-Prescott paper "made much of a splash... we were still much more in the standard IS-LM AS with expectations frame, to listen to that paper in the right way". Parkin also attended, and recalled (2002) that "The 1978 NBER conference presentation of "A competitive theory of fluctuations and the feasibility and desirability of stabilization policy" made virtually no impact. Everyone to whom I
spoke about the paper thought it was an interesting exercise that had no implications for either policy or future research. We were still wrestling with short-run non-neutrality. The idea that real disturbances could generate fluctuations and that money might be neutral after all was just too far off the radar screen for (almost) everyone. The same can be said about both the 1982 paper and the L-P [Long-Plosser] paper. People just were not thinking the way that Finn and Ed (or John and Charlie) were". Howitt, for his part, recalled (2005) "Yes, I was at the NBER conference, but I have no memory at all of the Kydland-Prescott paper. Nor do I recall ever seeing it referred to subsequently. My attention at the time was all on the issue of how monetary policy matters, and I probably saw this paper as being too far off topic to take seriously". When asked about his recollections, Blinder (2005)—as Howitt—said that while he was at the 1978 conference "given what has happened since, I am a bit embarrassed to confess that I have no recollection at all of the Kydland-Prescott paper", adding that as regards the impact of the NBER conference paper, there was "not much at the time…Hardly anyone appreciated where this might lead until after the 1982 Kydland-Prescott paper". Hall, who commented on the Kydland-Prescott paper at the conference recalled (2002) "My comments refer to the "equilibrium model" which was the way I thought about those models the time".

Nelson, for his part, had perhaps the clearest recollections of the NBER conference, while also making significant comments on the Kydland-Prescott paper. As he recalled (2002): "It was a great conference and I remember the general scene vividly (including noticing that Paul Samuelson was reading the St. Louis Fed weekly newsletter on money supply, and it had his name as addressee on it!). I can't really say that I recall the Kydland-Prescott paper making a splash, but we all had our personal reaction. Mine was that sources of lags they mention, particular time to
build, did not seem sufficient to account for the very great persistence of business cycle fluctuations as implied by their AR equation on page 171 [of the conference volume]. My comment is directed to the fact that the sum of coefficients is quite close to unity, so in light of the Dickey-Fuller problem of downward bias (which I was working on at the time in connection with the Nelson-Plosser paper), it is not clear that the sum is significantly below unity. The Nelson-Plosser view was that if it is unity than the cycle is not just long-lived but possibly not stationary. Of course, what we argued in our paper was that the unit root--sum equal to one--could not be rejected, so detrending may be entirely artifactual and the trend process may account for variation that the Kydland-Prescott's simple linear detrending attributes to the cycle. Indeed we argued that perhaps all the variance in output is attributable to trend, leaving no transitory "cycle" to explain.

Above, the term *enigmatic* was used to describe the story of the 1978 Kydland-Prescott paper. Close inspection of the comment by Taylor on the paper published in the conference volume reveals the following anomaly. In his comment, Taylor wrote (1980, 193): "Kydland and Prescott build their equilibrium business cycle model upon the assumption of utility maximization. That is, they posit a representative household utility function which depends on consumption, leisure, and *government expenditures*, and they assume that households maximize this utility function subject to budget constraints" [my emphasis]. However, in the utility function in the version of the paper as published in the conference volume, *government expenditures do not appear* (1980a, 174,177). When asked about this, Kydland replied (2006): "We wrote a paper for the NBER conference containing a business cycle model (not unlike, as I recall, that in the paper I had written up in preparation for my "job talk" -- that is, converting my one-year visiting position to permanent -- at CMU that same spring)
along with an application to public finance. That application would have shown that fluctuation in the desired provision of public goods, combined with cyclical fluctuations otherwise, implied that the fluctuation ought to be picked up primarily by changing government debt and not by changing tax "rates". In other words, the paper had somewhat of a dual focus (often not a good idea), as reflected also in its title. After the conference, the editor (Stan Fischer, as I recall) told us the paper was too long for the volume and had to be cut. So we more or less omitted the portion emphasizing cyclical public finance (with a heavy heart, because we thought the message was really interesting and innovative). Of course, with that emphasis removed, there was no longer any point in keeping government purchases in the utility function”.

But more is involved than simply the elision of material from the version of the conference paper as presented by Prescott, and commented upon, by Taylor, among others. There are, in fact, *five versions* of the 1978 Kydland-Prescott conference paper, four drafts (1978a, b, c, d), and the published version (1980 a). Finn Kydland has kindly provided the author with some of these drafts. Kydland has also explained--in an extensive interview (2005, interview)-- their significance and relation to the published version of the conference paper, and the relationship of the conference paper to *other work* in the context of what can be called the "time to build research program", all within the framework of the *overarching* Kydland-Prescott research program.

According to Kydland (2005, interview), the "first draft" of the Oct. 1978 NBER paper, that is to say "time to build", as Lucas put it (2005), was a draft paper by Kydland and Prescott entitled "Persistence of unemployment in equilibrium", dated 19 April 1978 (1978 a). This draft was the basis for the "time to build" research
program, on Kydland's view (2005, interview), *as it was the first modern real business cycle paper, in that it was quantitative and encompassed models of people and businesses* (1978 a, 5). The catalyst for this paper was that both Kydland and Prescott were "bothered by 'persistence' based upon rigidities and adaptive expectations" (Kydland 2005, interview). The draft included an explicit time to build feature in "the basic model" (1978 a, 5-6) and a quadratic utility function, in addition to the possibility of monetary shocks (1978 a, 9).

Significantly, the earliest version of the Hodrick-Prescott paper (1978) is cited; the importance of this paper to the story overall will be dealt with below. As Kydland and Prescott wrote (1978 a, 2): "It has generally been the belief that prices and output move procyclically (See Lucas (1977) and Sargent (1978)). Recent empirical results by Hodrick and Prescott (1978) for the period 1947-77 indicate, however, that deviations from trends the two series are negatively correlated. This is true even when the years after 1973 are excluded. This observation would hardly be consistent with monetary shocks being the major cause of fluctuations. In view of this, it appears appropriate to look to real shocks, such as technology shocks, as an explanation of this perhaps surprising phenomenon".

According to Kydland (2005, interview), the April 1978 draft is linked to both the 1978 Kydland-Prescott NBER paper and to the paper by Kydland entitled "Analysis and policy in competitive models of economic fluctuations" (1980). A notable feature of the April draft was that according to the title page it was "preliminary and incomplete" and comprised "Background material for GSIA Seminar, April 19th, 1978". According to Kydland (2005, interview), he gave this seminar, as he was being considered at the time for a tenure track position.
Interestingly enough, according to Kydland (2005, interview), Black read the paper and mailed a list of questions to him.

Most importantly, however, is the fact that the model presented in this April 1978 draft was the basis for the model that appeared in the published version of the 1978 Kydland-Prescott paper, as will be shown below. There is no government expenditure in the utility function in the April 1978 draft, and "the driving terms of the model are productivity and possibly tastes" (1978 a, 11). The model is in a "dynamic competitive equilibrium" (dynamic general equilibrium) framework (1978 a, 11-13). The "draft" entitled "On the possibility and desirability of stabilization policy", actually had two draft versions, the first, a "preliminary" version (1978 b), dated September 1978, and an amended version of the same title actually presented at the NBER conference, also dated September 1978 (1978c). According to the title pages, both were "Prepared for the NBER conference on rational expectations and economic policy, October 13th-14, 1978". The major differences between the preliminary version (1978b) and that submitted to and presented at the NBER conference by Prescott (1978c) were the inclusion, in the latter, of a number of important illustrative examples, and the addition of two figures. The final version was dated: "October 1978, revised Dec. 1978" (1978d). According to the title page, the title was changed to "A competitive theory of fluctuations and the feasibility and desirability of stabilization policy". This was also the title of the paper as published in the conference volume.

Interestingly enough, an additional joint Kydland-Prescott paper that input somewhat into their NBER paper was a paper originally dated "November 1977, revised May 1978", entitled "Rational Expectations, Dynamic Optimal Taxation, and the Inapplicability of Optimal Control" (1977b). This paper later appeared, with a
slightly different title, and in revised form, in *Journal of Economic Dynamics and Control* (1980), but it was cited by them, with its *earlier* title in the Oct. (Dec.) 1978 "revised" version of "A Competitive Theory" , with the last paragraph of the intial "Dynamic Optimal Taxation" paper actually becoming the basis for a "footnote" in the "Competitive Theory" paper.

There are a number of differences between the amended September 1978 version (1978c), which on the basis of Taylor's comment and Kydland's recollection as cited above, *was that presented at the conference*, and the October-December 1978 revision (1978d) -- both formal and substantive. Moreover, there is one very minor-- albeit significant--difference between the October-December 1978 revision, and the version published in 1980 (1980 a), in the form of an additional reference in a note in the text, as will be seen below.

Moreover, the October-December 1978 revision contained an abstract, absent from the Sept. 1978 amended version, which read as follows: "A competitive theory combining elements of Lucas' (1972, 1975) monetary shock theory with a model of equilibrium capital accumulation, under uncertainty is developed. The model assumes that multiple periods are required to build new capital goods. The resulting equilibrium process displays both the observed co-movements of economic aggregates and observed serial correlation of real output from trend. A conclusion is that the tax rates should be constant over the cycle. This does not minimize fluctuations but does minimize the burden of financing government expenditures".

The difference between the references in the September 1978 amended version, October-December 1978 revision, and the 1980 published version include citation, in the *latter versions*, of published papers by Debreu (1954) and Friedman (1948), working papers by Brock (1978) and Prescott and Mehra (1978), and two 1978
Carnegie-Mellon working papers by Kydland and Prescott, including their paper "Persistence of unemployment in equilibrium". Moreover, to the note (1978 d, 10, 5; 1980 a, 175, note 5) referring to the passage "Ours is a competitive theory which combines the Lucas (1972) monetary shock model with a model of capital accumulation in an environment with shocks to technology", the following was added in the 1980 published version: "Black (1978) has argued that real factors can explain aggregate fluctuations".

But the crucial difference between the September 1978 amended version (1978c) and the October-December 1978 revision (1978d) and 1980 published version (1980a) can be seen in the model in the latter versions; a model which emanates from the Kydland-Prescott "Persistence" paper of April 1978 vintage (1978a), that is, in Kydland's view, as cited above, the first modern real business cycle paper.

The post transitional phase: fluctuations, empirical investigation, time to build and aftermath

In his paper "Business cycle research: methods and problems" (1998), Prescott included a section entitled "history and overview of business cycle theory". In it he said (1998, 5-6) that "Lucas defined business cycles to be recurrent fluctuations of output and employment about trend. He wrote that the key business facts were the co-movements of economic time series". Prescott continued "Hodrick and I (1980) developed a statistical definition of the business cycle component of an economic time series... Exploiting Arrow-Debreu language, recursive methods, and computational methods, Kydland and I (1982) derived the implications of growth theory of business cycle fluctuations".
In correspondence, Prescott later provided retrospective assessments of the history and the impact of the 1978 and 1982 "time to build" papers. Because of their significance, they are cited at length here. He wrote (2001, 2002) "the 1978 paper did not have much of an impact. In fact Finn and my 1982 paper did not have much of an impact. At the time...the only person who thought it was important was Bob Lucas. The big break in Finn and my thinking...came in 1980 when [we] decided to begin with the growth model with the leisure decision endogenous... [and] to use the growth model to study fluctuations. The beauty of growth theory is the connection between it and the system of national accounts. Restricting our linear-quadratic economy so that it behaved in the same way as the growth model when not to distant from the steady state seems little in retrospect, but was a major [breakthrough]... At the time we...were convinced that monetary shocks were the cause of business cycle fluctuations… Finn and my paper forced to change my mind. Prior to writing the paper, and finding that the productivity shocks were of the right magnitude and persistence, I was certain that monetary shocks were the factor giving rise to business cycle fluctuations and the problem was to find the propagation mechanism for these shocks. We were searching for a propagation mechanism for monetary shocks along lines suggested by ... Frisch many years before. At the time...Black and...Plosser...[were] the only people I know who would argue that real shocks are all important... Finn and I (1982) were surprised when we found that persistent changes in the factors that affect the steady state of the deterministic growth model gave rise to business cycle fluctuations. Finn and I in this paper broke a taboo against general equilibrium in macro".
In order to fully appreciate Prescott's recollections as cited above, the evolution of the 1982 Kydland-Prescott paper, in its variorum drafts, has to be dealt with. Moreover, as he is specifically mentioned in the recollections of Kydland and Prescott, and cited in the "time to build" papers, the role of Fisher Black in the story also has to be dealt with (that of Plosser and the development of the Long-Plosser approach will be considered in a separate paper). But before doing this, we have to consider the development of the 1978 versions of the Hodrick-Prescott paper and its impact on the Kydland-Prescott approach to aggregate fluctuations.

**Variorum Drafts of Hodrick-Prescott**

The importance of the Hodrick-Prescott paper in the evolution of the Kydland-Prescott approach cannot be overstressed. Indeed, as shown above, it was cited as early as the April 1978 draft of the NBER "time to build" conference paper, and also in the 1980 published version of this paper. The first draft of Hodrick-Prescott paper was originally entitled "Money and business cycles in dynamic competitive equilibrium". This was the title that appeared in the published program of the Econometric Society meeting (1979, 245). The first version of the Hodrick-Prescott paper was presented on Wednesday afternoon, 30 August 1978. The chairman of the session "Macroeconomic implications of rational expectations" was McCallum; the discussant was Kmenta. Other papers in the session were by Evans and Sargent. But the title of the paper actually presented at the session was "Postwar U.S. business cycles: a descriptive empirical investigation" (Hodrick-Prescott, 1978). As the Hodrick-Prescott paper is a central component of the "time to build approach", its origin and development is an important element in the Kydland-Prescott research.
program overall. In recent correspondence, Hodrick provided his detailed recollections. He wrote (2005 a, b):

"The genesis of the Hodrick-Prescott paper occurred when [Ed] asked me what were the stylized facts regarding the velocity of circulation of money over the business cycle. We started thinking about this issue and submitted a preliminary idea for a paper to the Econometric Society meetings. What we realized when we started working on the project was that the only way people had to describe the cyclical properties of any data series was the NBER's specific cycle and reference cycle terminology, which was not amenable to time series analysis. We needed to develop something new.

We decided that we needed a way to decompose any time series into a trend and cyclical component, and we wanted the trend component to be able to change over time, but not too much. All that we were able to accomplish by the time of the Econometric Society meetings was the HP filter and some descriptive statistics… The "Money and Business Cycles title was what we hoped to deliver, and the "Post-War..." was a descriptive title of what we actually delivered.,,

I presented the paper, and Jan Kmenta was the discussant. I remember him being not particularly positive about the paper, and I was at a loss for how to respond. Tom Sargent came to my defense with an eloquent speech about the importance of this type of work.

There is another interesting story about the paper. We first submitted the paper to the AER, and it was rejected because the referee wanted us to use formal Bayesian smoothness priors. Around the time of the rejection, Ed was talking at the Carnegie-Rochester Conference to Bill DeWald, who was the editor of the Journal of Money, Credit and Banking. Bill said that he would like to have the paper reviewed for the
JMCB, and promised to get a good referee. We submitted the paper and received a rejection because DeWald's referee didn't like to paper, at all.

The referee was Milton Friedman! He indicated this in his report stating "I have no reason the authors should not know my opinion of their paper." He didn't like the idea that we were doing "measurement without theory." He thought that this issue had been settled by the Cowles Commission in the 1950s-1960s. I wish I had that report, but I don't think I do. It always seemed ironic to me that Friedman had made part of his reputation on the Monetary History of the US, and later developed theory to support the measurements done in that book.

By this time, Ed had left CMU for Minnesota, and I was on my way to Kellogg's finance department at Northwestern. We planned to revise the paper and resubmit it, and I spent a week one summer at Minnesota in the mid-1980s doing some revisions. But, we never resubmitted it".

Hodrick continued (2005b):

"In early 1996, I got a call from Steve Cecchetti, who was editing the JMCB. He asked if he could publish the HP filter paper, and after talking with Ed, we said yes. We agreed that Ed and I would update the descriptive statistics, but we would not modify the paper with lots of references to the intervening literature. Steve did not know the history of the paper and was shocked by the Friedman story."

Now, this is not the place for detailed discussion of the differences between the August 1978 version of Hodrick-Prescott and the November 1980 revision. However, it should be noted that the title itself was changed by elision of the word "descriptive". In the 1978 version, the April 1978 Kydland-Prescott "Persistence" paper is cited, while mention of this paper is elided in the November 1980 version of Hodrick-Prescott. In the introduction to the 1978 version, both stochastic monetary and real
shocks are mentioned, and "correlation between deviations from trend rate of inflation and deviations from trend real output" are analyzed (1978, 2-3). A "two-shock theory of the business cycle is also presented" (1978, 3; section 4). According to the 1978 version (1978, 3) "it appears reasonable to conclude that an econometric analysis using dynamic equilibrium theory which is structured around real supply side and monetary or inflation shocks will one day explain a large part of the aggregate economic fluctuations including the persistence of deviations from trend experience by the U.S. ". In the 1980 version, emphasis is focused on the interaction of growth and cycle in investigating "aggregate economic fluctuations". According to this introduction (1980, 2) "At a substantive level our primary objective... is to examine the magnitudes and stability of covariances between various economic time series and real output and the autocovariances of real output". The 1980 version concludes by saying (1980, 23) "In this article no explanation of the cyclical regularities is offered. We think such an explanation can be provided only within the context of a well specified economic model. We do think it appropriate, however, to study the observations prior to theorizing".

In June 1980, Nelson wrote Hodrick regarding the Hodrick-Prescott paper, referring to the August 1978 version, and asked for information as to its revision or publication status, so as to be able to cite the paper in the "Two Charlie's paper " then he was then putting together with Plosser. In November 1980, Prescott sent Nelson a copy of the November 1980 revision, and in a covering letter wrote "Enclosed is the revised version ...subsequent to our telephone conversation I learned that our method has a long history of use. We do agree that the method of decomposing high and low frequency variation matters. The question is whether first differencing is a good procedure for studying cyclical fluctuations or Bob's and my procedure or some other
procedure. My view is that there are many ways to look at the data and theory is needed in the selection of the way" (Prescott, 1980 b). And it is to the application of that theory in the form of the 1982 Kydland-Prescott paper that we now turn.

**Time to build and competitive models of economic fluctuation**

The manuscript of the 1982 Kydland-Prescott paper was received by *Econometrica* in January 1981, and the revision was received a year later, in January 1982 (1982, 1369). Publication of the paper was announced in the list of accepted manuscripts that appeared in the July 1982 issue of *Econometrica* (1982, 1085), and the paper eventually appeared in the November 1982 issue. Much as been written about the impact of this paper in the form of the subsequent Kydland-Prescott Real Business Cycle research program that emanated from it. However, the evolution of the 1982 paper itself--in terms of its variorum drafts from 1977 onwards--has not been dealt with. Moreover, the "sting in the tail" of the paper, that is, according to Kydland and Prescott, the need for "methods... to analyze policy rules in competitive environments" (1982, 1369; citing Kydland, 1980) has not been noted, or discussed, and it is to these aspects of the 1982 paper that we now turn.

As in the case of their 1978 NBER "time to build" paper and its final version as published in 1980 (1980 a), the evolution of the 1982 Kydland-Prescott "time to build" paper is characterized by a number of draft versions and the final version, as published in 1982. Now, the link between the April 1978 Kydland-Prescott paper "Persistence of unemployment in equilibrium" and the 1982 "Time to build" paper can be seen in what Kydland considers(2005) to be the first "formal draft "of the 1982 paper, that is, the October 1979 revised draft version entitled " Time to build and equilibrium persistence of unemployment". According to Kydland, this draft was just circulated for comment, "probably to Lucas". The term "equilibrium persistence of
unemployment" remained in the title in the Oct. 1979, September 1980 and December 1980 versions, and was changed to "aggregate fluctuations" only in the Dec. 1981 revision, sent for publication.

What is also important about the October 1979 revised version is that it contains handwritten amendments and additions attesting to the interaction between Prescott, who was then at Minnesota and Northwestern, and Kydland at Carnegie Mellon. For example, Prescott change the original term "the relative demand shift "to" productivity shock" (1979, 3); added "adjustment costs" 1979, 7; 1982, 1348), and a footnote regarding "beginning of period stocks (1979, 9; 1982, 1349). But more important was the utilization in this version, of an exponential "constant relative risk aversion utility function" (1979, 12), which is made quadratic in the September 1980 version (1980b, 12). According to Kydland (2005, interview), the September 1980 version should be regarded as the first complete version; it was issued as a GSIA working paper (No. 28-80-81) and sent to Cornell for a seminar given by Kydland (2005, Nobel autobiography). Moreover, in Kydland's opinion (2005, interview), the September 1980 version also contained the central message of the 1982 Kydland-Prescott paper in the form of the sentence "Our approach integrates growth and business cycle theory" (1980 b, 2; 1982, 1345); this, according to Kydland (2005, interview) and Prescott (2004, 376, note 1) follows from the 1978 and 1980 versions of the Hodrick-Prescott paper. Interestingly enough, the December 1980 revision of "time to build" was submitted to *Econometrica* under the title "Time to build and the persistence of unemployment". The change in title in the Dec. 1981 revision sent to *Econometrica* was to make it, in Kydland's words (2005, interview), "more general".
Finally, as regards the influence of Black, while Prescott wrote in correspondence (2002) that he "did not influence my thinking on business cycles", Black's November 1979 MIT working paper entitled "General equilibrium and business cycles" is cited in both the December 1980 and December 1981 revisions of the Kydland-Prescott "time to build paper", and in the 1982 published version. Moreover, an earlier 1978 version of Black's working paper was cited in the drafts and published version of the Kydland-Prescott NBER conference paper.

There are five versions of Black's paper. The original version appeared in April 1978 as an MIT-Sloan School Working paper, which was first revised in September 1978 and later revised in November 1979. A 1978 version of Black's paper was not cited in the Kydland-Prescott NBER conference paper draft dated September 1978, but was cited in the Kydland-Prescott draft dated Oct. 1978 and revised Dec. 1978, and also appears in the Kydland-Prescott paper as published in the NBER conference volume (1980 a, 196). Interestingly enough, the third version, that is the Nov. 1979 revision of Black's 1978 paper, was cited by Kydland-Prescott as early as in the September and December 1980 revisions of "Time to build", and also in the published version of the "Time to build" paper (1982,1369). The fourth version of Black's paper appeared in August 1982, as an NBER Working paper. The fifth and final version was published in Black's 1987 book.
"Loose Ends": "Competitive models of economic fluctuation", "The role of money in a business cycle model", and interim summary

In the section of his Nobel autobiography entitled "Loose Ends", Kydland recalled that in 1980 he "wrote a first draft of a paper in which I focused on the role of money for the business cycle"; a paper which, although revised a number of times, and submitted to various journals, "remains unpublished". Kydland did not cite the title of this paper in his Nobel piece, but acknowledged (2005, interview) that he was referring to his May 1980 draft of "Analysis and policy in competitive models of economic fluctuations", which was cited at the very end of the 1982 Kydland-Prescott paper. Moreover, Kydland asserted (2005, interview) that his 1980 paper had a "double focus" problem, which was rectified only by its revision and transformation into his December 1987 paper "The role of money in a business cycle model". In this December 1987 revision, parts of the April 1981 draft of his "Analysis and policy in competitive models of business fluctuations" (e.g. 1981, 21-30), formed the basis for an analysis of the role of money in a "real model of aggregate fluctuations" (1987, 3-10). The final version of Kydland's May 1980 paper was issued as a Minneapolis Fed discussion paper, in December 1989.

Three additional points that have to be dealt with are: (i) the role of Solow's 1956 and 1957 papers; (ii) the influence of Lucas's 1972 paper "Expectations and the neutrality of money"; and (iii) the impact of Lucas's 1977 paper "Understanding business cycles" on the Kydland-Prescott program. With regard to Solow's papers, it is interesting to note that from the October 1979 draft of "Time to build" to the 1982 published version of the paper, neither of Solow's papers is cited. According to Kydland (interview, October 2005), while they are not mentioned because they were not directly utilized, the influence of the Solow papers was "at the back of their
minds”. This is clearly evident in Prescott's Nobel lecture (2004, 379). With regard to Lucas (1972), while it is cited in both the October 1979 and December 1980 versions of "Time to build", it is not cited in the 1982 published version. Despite this, Kydland asserted (interview, October 2005) that it was very influential on his thinking, and that he had originally been introduced to it in Lucas's Spring 1970 class on "Economic fluctuations" were an early version was taught. Kydland's explanation (interview, October 2006) of why it was not cited in the published version of "Time to build" is that this version put "monetary shocks" to one side, and concentrated on "real shocks", whereas the October 1979 version included a shock that "mimics the effect of a Lucas (1972) monetary shock" (1979, 2), and the reference was brought over into the December 1980 version, but such as such a shock did not appear in the published version, reference to Lucas (1972) was elided.

More important, however, is the role of Lucas's "Understanding business cycles". The paper was originally presented at the June 1976 Kiel Conference on Growth without Inflation, and according to the introductory note in its published version, was revised in August 1976 (1977, 7). While there are a number of textural differences between the original and revised versions, the central message of the Lucas paper, as seen by Kydland, already appeared in the June 1976 version. Interestingly enough, while the June 1976 version is not mentioned, the 1977 published version of the paper is cited a number of times in the Kydland-Prescott April 1978 draft "Persistence of unemployment in equilibrium", which is the first draft of the later "Bald Peak" paper (Lucas's "Time to build" paper), or as Kydland put it, the first draft of "Time to build" (interview, October 2005). Moreover, as Kydland said (interview, October 2005), they had made Lucas (1977) operational, and thus brought about a change from the methodological approach they took in their
April 1978 "Persistence" draft, to that in their "Time to build" draft of October 1979, which, in Kydland's view, forms one research program. In the October 1979 version of "Time to build", according to Kydland (interview October 2005), Lucas's 1977 statement that "one exhibits understanding of business cycles by constructing a model (Lucas's emphasis) in the most literal sense: a fully articulated artificial economy which behaves through time so as to imitate closely the time series behavior of actual economies" (1977, 11), became one of the methodological precepts for the 1982 "Time to build" approach.

There are still elements in this story that remain to be told, including the influence of the Brock-Mirman papers, and the ostensible impact of work of Shoven and Whalley (according to Heckman and Hansen); and the relationship between the Kydland-Prescott story and the Long-Plosser story.

This paper, therefore, only reflects work in progress, since there is more material regarding the evolution of the Kydland-Prescott research program still extant. These include unpublished correspondence and comments on papers by Kydland and Prescott, correspondence regarding the relationship between the Kydland-Prescott approach and that of Long and Plosser, and referees reports on "Time to Build". This material has only recently been collected by the author, and remains to be collated and integrated with what has been presented above. Moreover, recollections and documents provided to the author by Brock, Radner, Shoven, Whalley, and Plosser, among others, remain to be assessed and integrated into the story. There is, in fact, much more work to be done so as to tell the complete story of the evolution of the Kydland-Prescott research program.
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